Exhibit 10:

Dr. Kelly Socia Rebuttal Report

A Rebuttal to the Arguments Raised by

Lovell, Goodman-Williams, Salter, Dare, Bennetts and Rennie

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Note: The analyses and views represented in this document are those of Kelly M. Socia, Ph.D., and do not necessarily reflect those of the University of Massachusetts, Lowell.

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Purpose of the Current Report

The current report is a rebuttal that largely focuses on the arguments raised by Lovell (*Lovell Report*), Goodman-Williams (*Goodman-Williams Report*), and Salter (*Salter Report*). While some of the points raised in these expert reports are compelling and sympathy producing, it is my opinion that most of the content in all three reports is largely irrelevant to the issues in the current *Does III* case. Thus, I respectfully disagree with many of the experts' conclusions.

Below, I begin by summarizing the key conclusions from my initial report (*Socia Report*), as a baseline to compare the other reports to. I then present a summary of the opinions in this rebuttal report. I then present Lovell's own statements/opinions from the *Lovell Report* and respond to these opinions. Following this, I do the same for the statements made by Goodman-Williams, and again for the statements made by Salter. I only briefly address the statements of Dare, Bennetts, and Rennie at the end of this report, as those statements are not grounded in research and are more in the form of personal opinions. I incorporate by reference the statements made in my earlier declarations.

Summary of My Opinions

First, a major theme of Defendants' experts' reports is that, due to underreporting, the actual rate of sexual offending is higher than the rates of arrests/

charges/convictions that are captured in official recidivism statistics. That is uncontroversial. Defendants' experts generally agree that actual offense rates are unknown and unknowable. Yet at the same time, they suggest that the Court base its decision on that unknown and unknowable data. It is precisely because data about unreported crime is unreliable that the field of criminology has for decades relied on official crime statistics. These data involve large samples and are reported much the same way year after year, which allows researchers to identify variables that have an effect on, or are affected by, the official crime data.

Second, Defendants' experts inconsistently argue that, on the one hand, the actual offense rates are unknowable and, on the other hand, that those rates are high. The studies that Defendants' experts cite, however, rely on measures and/or samples that are simply not reflective of (i.e., generalizable to) the sexual recidivism or reoffending rates of *current registrants* in the United States. The reasons for why those prior estimates cannot be generalized to the population in question are covered below when I discuss the problems of the 'dark figure' studies, the sexual assault kit (SAK) studies, and other research highlighted in the Lovell, Goodman-Williams, and Salter Reports.

Third, Defendants' experts repeatedly conflate repeat offending with reoffending after a criminal sanction. The question is not what percentage of people who commit sexual offenses do so more than once *before* being arrested, charged,

or convicted (as Defendants' experts suggest). Rather the question is to what extent do people with convictions for past sexual offenses (registrants) reoffend – and how do those rates compare to offense rates in the general population (i.e., people not on the registry). Defendants' experts do not address these questions.

Finally, Defendants' experts do not rebut the key conclusions in my report. They concede that 90-95% of reported sex crimes involve individuals who *do not* have prior sex crime convictions (Salter, p. 9-10). They do not dispute that five-year recidivism rates *based on rearrests* are between 5-15% (Lovell Report, p. 5, citing Lussier et al. [2023] (13% over 5 years), and Alper and Durose [2019] (8% over 9 years)). They recognize that contrary to popular belief – most sex crimes are committed by people known to the victim (Lovell Report, p. 9). Finally, they do not respond to the point that the public is intensely stigmatizing towards registrants, and they fail to address the point that non-sexual SORA-compliance offenses (like failure to register) do not correlate with sexual recidivism.

Defendants' Experts Do Not Rebut, but In Fact Support, the Key Conclusions in My Report

In my initial report (Socia, pp. 2-3), I presented evidence to support the following conclusions (shortened and paraphrased) below:

> **Finding #1**: Approximately 90-95 percent of sex crime arrests involve individuals who *do not* have prior sex crime convictions, and thus who are not listed on a sex offender registry at the time of the offense. This finding is *not*

an effect of registries, because it was also true before registries came into existence: more than 90 percent of sex crime arrests were committed by first-time offenders both before *and* after registry laws were passed.

Finding #2: The widely held public belief in "frightening and high" sexual recidivism rates is not supported by research evidence. The best estimate of the official sexual recidivism rate (i.e., rearrests) among the population of individuals listed on the sex offender registry, or with a prior sex offense before registries existed, is between 5% and 15% (rearrest for a sex crime) within the first five years of conviction/release. If *reconviction* is used as the indicator of recidivism instead of rearrest, the rate is even lower. This recidivism rate—for being re-arrested or re-convicted of the same kind of offense—is lower for sexual offenders (and in most cases far lower) than for all other categories of offenders (like non-sexual assailants, armed robbers, drug dealers, or those who commit property crimes, etc.) but one: only murderers have a lower recidivism rate than sexual offenders.

Finding #3: In addition to incorrect beliefs about recidivism rates, the public believes other myths regarding individuals listed on the registry. These include beliefs that *most* registrants have stranger victims, are sexual predators and/or pedophiles, and are at high risk to commit sex crimes involving strangers, kidnapping, and victims of all ages. None of these beliefs are supported by research evidence on the criminal history of people convicted of sexual offenses, nor research on the recidivism rates of those on the registry. Despite the research consensus that registry laws don't reduce sexual recidivism, the public incorrectly believes that they do work.

Finding #4: Perhaps because of these widely held but incorrect beliefs about registrants and sex crimes generally, the public is intensely stigmatizing towards anyone listed on the sex offender registry. This stigmatization of those on the registry, in turn, hurts the chances of successful reentry and rehabilitation, especially as it relates to employment and housing outcomes.

Finding #5: There is no consistent evidence that a failure to register conviction is associated with an increased likelihood of sexual recidivism.

Defendants' expert reports agree with parts of my initial report, and do not

present evidence that rebuts any of these five conclusions. Rather, much of their

reports discuss matters that are largely irrelevant to the issue of sexual reoffending (or sexual recidivism) among people with prior sex crime convictions, including Michigan registrants.

Statements Expressed by Lovell and My Responses

Starting on page 2, Lovell states a set of opinions. Rather than paraphrasing, I directly quote these opinions, and then immediately respond to them:

> **Opinion:** *Research on recidivism and repeat offending (also referred to as serial offending) emphasizes the importance of operationalizing these terms.*

Lovell provides five separate definitions for criminal recidivism, sexual recidivism, repeat sexual offending, risk of sexual recidivism, and risk of sexually reoffending. However, for any such definitions to be useful, one also must know (1) the population being studied (here people *convicted* of sex offenses); (2) the length of time being covered (e.g., 3-year versus lifetime rate); and (3) the time window (e.g., since release from incarceration, or after 5 years offense free in the community).

Any statements about the recidivism or reoffending rates of "sex offenders" as a group is inherently flawed, because the rates vary tremendously depending on the population studied. For instance, research based on sexual psychopaths (e.g., Weinrott and Saylor, 1991) will provide quite different results from research based on individuals who have a single sex crime conviction (see Mathesius and Lussier,

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2014), or individuals with non-contact child pornography convictions (see Bourke and Hernandez, 2009). Using risk estimates taken from research on very high-risk populations provides little or no useful data to describe the more than 50,000 people on the Michigan registry. Similarly, providing information about the risk of offending sexually among people who have not yet been convicted of a sex crime tells us little about the risk of future sexual *reoffending* or *recidivism* (using whatever definition you wish) among people who *have* been convicted of a sex crime.

Second, identifying follow-up periods and time windows is important because it provides some context to the risk being estimated. Research is clear that following a sex crime conviction, most sexual recidivism occurs within the first few years of returning to the community (e.g., Alper & Durose, 2019; Black, et al., 2001; Harris and Hanson, 2004). Thus, while longer periods will naturally show higher *overall* recidivism estimates, it is only *because* they are covering a longer period of time (including the initial shorter period). This means that comparing a longer follow-up period to a shorter follow-up period does not directly inform us on how the risk of recidivism (or reoffending) *changes* over time.

Third, we know that the longer people remain offense-free in the community following an initial conviction/release, the less likely they are to be re-arrested or reconvicted for a new offense (Harris and Hanson, 2004). As noted earlier, most sexual recidivism occurs soon after returning to the community, and this risk goes

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down dramatically over time as individuals remain offense-free. This also means

that following an initial conviction/release, the risk of recidivism (or reoffending)

will be *different* during years 0-3, years 3-6, years 6-9, etc. So even though a 15-year

cumulative recidivism rate is higher than a 3-year cumulative recidivism rate, the

rate of recidivism in later years (e.g., years 13-15) for a particular population is going

to be much, much lower than that population's rate in the initial years (e.g., years 1-

3). Lovell (and Defendants' other experts) repeatedly cite studies of a single narrow

population (usually high-risk and usually in the first years after release) and then—

with no science to back them up—extrapolate the results to all people who have been

convicted of sex offenses.

> **Opinion:** *The dark figure of crime is a well-known criminology principle that most crime is not reported. The actual crime rate is the "dark figure."*

This definition is not accurate. The 'dark figure' is more typically referred to

as the *difference* between the actual crime rate and the crimes known and recorded

by the justice system (see Biderman & Reiss, 1967). In other words, the 'dark figure'

is the crime that occurs but *does not* get reported to and recorded by the police.

> **Opinion:** *The term sexual recidivism cannot be used interchangeably with repeat sexual offending. The former is smaller, easier to measure, and has less measurement error. Comparatively, the latter is much larger (how much larger is a matter of much debate, the dark figure of sexual recidivism), difficult to measure, and has larger measurement error.*

I agree that sexual recidivism and repeat sexual offending are not interchangeable, and that sexual recidivism is indeed 'smaller, easier to measure, and has less measurement error' than repeat sexual offending. I also agree that the unknown 'dark figure' is difficult if not impossible to measure. Indeed, one of the reasons most researchers rely on official crime statistics and recidivism data is that it is so much more reliable than data sources that attempt to measure unreported crime.

> **Opinion:** *National prevalence estimates of sexual victimization, based on representative samples of the United States (U.S.) population, including both reported and unreported crimes, indicate that one in five women and one in 38 men have experienced a completed or attempted forced penetration in their lifetime.*

The exact estimates of sexual victimization among the U.S. population varies based on the sample, the definitions of sexual victimization used, and other methodological choices. For example, in some studies sexual victimization estimates include incidents involving unwanted sexual contact such as kissing, verbal harassment in public, and/or verbal threats or 'sexual coercion'. (For discussion, see Fisher and Cullen, 2000; Gilbert, 1997.)

Depending on the methodology used, there is a broad range of estimates of lifetime sexual victimization rates in the general population, which reduces the certainty of any given estimate. For instance, even when lifetime estimates of completed or attempted rape among women have similar *overall* estimates between studies, such as 17.6% (Tjaden and Thoennes, 1998), compared to 19.1% (Smith, et

al., 2017), *within* such studies there is a broad range of estimates based on the subpopulation being considered. Tjaden and Thoennes (1998) note that the lifetime prevalence of rape in their sample varied by race, ranging from 6.8% for Asian/ Pacific Islander women, up to 34.1% for American Indian/Alaska Native women. Smith and colleagues (2017, p. 18) found wide geographic differences, with state-level estimates of lifetime rape victimization (completed or attempted) ranging from 12.2% to 26.4%. Estimates of childhood sexual abuse have even broader ranges, with one meta-analysis presenting an estimation range of between 2% to 62% (Bolen and Scannapieco, 1999; see also Finkelhor et al, 2015).[1]

But even taking the above estimates of the lifetime risk of 'completed or attempted forced penetration' at face value, the *victimization rate* of the general population cannot be clearly connected to either (1) the *sexual recidivism* (or sexual reoffending) rate or risk posed by individuals who have already been convicted of a sex crime and released back into the community, or (2) whether registries reduce *sexual recidivism* (or sexual reoffending) by that population. As noted below, Lovell

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[1] The dramatic 62% estimate comes from a single study conducted by Wyatt (1985), who surveyed 248 women in Los Angeles County between the ages 18 and 36 for a wide range of childhood sexual abuse events (e.g., unwanted kissing, solicitation, exposure, fondling, oral sex, intercourse). Thus, the estimates from this study are already almost 40 years old.

relies on this high victimization rate to conclude that repeat sexual offending is common, but as I will explain, this cannot be extrapolated to the population at issue here: individuals with prior sex crime *convictions* (registrants).

> **Opinion:** *Simplistically, these high estimates [of sexual victimization] imply: (i) a sizable portion of the population, mostly male population, are sexual offenders or (ii) repeat sexual offending is common. Research suggests that scenario 5a(ii)—repeat sexual offending— is more likely to be the key driver of high prevalence rates…*

There are numerous assumptions embedded in these statements that need to be unpacked, given that the relevant question for this litigation is how *convicted* people compare with people who have not yet been caught, convicted, and punished. Only the former are on registries. The question of: a) whether sexual victimization is mostly caused by repeat offending, is distinct from both the questions of: b) whether sexual victimization is mostly caused by people on registries, or c) whether people on registries commit many acts of repeat offending. Lovell largely argues about the first question, while missing the latter two questions, both of which are more relevant to the present case.

First, research shows that the vast majority (90-95%) of sexual crimes that are reported annually and result in arrest are committed by people who *do not* have a prior sex offense conviction (Sandler et al, 2008; Levenson and Zgoba, 2016). This suggests that it could well be true that "*(i) a sizable portion of the population, mostly*

male population, are sexual offenders." Indeed, as noted later in this rebuttal, multiple studies are cited by Lovell and other experts demonstrating the high rates of self-reported sexual offending (and reoffending) among sample groups who do not have a sex crime conviction (e.g., college students; enlisted Navy personnel; Lisak and Miller, 2002; McWhorter, et al., 2009). Sexual offending encompasses a wide range of behavior, and it seems possible that a sizeable portion of the male population are engaging in acts such as unwanted sexual contact, particularly when alcohol is involved.

Second, with respect to unreported crime, if – as Lovell suggests – repeat sexual offending is the key driver, it follows that people *not yet* convicted of sex offenses (who are responsible for almost all sex crimes [Sandler et al., 2008; Levenson and Zgoba, 2016]) are committing multiple sex offenses *before* getting caught. It does not follow that people with past convictions are reoffending at the same rate *after* being sanctioned, because if this were the case, then one would expect to see much higher rates of rearrest among this sample than are found in most recidivism studies, along with sustained high rates of rearrest over a long follow-up period. But research suggests the opposite: that after a conviction, recidivism (and by extension, reoffending) is most likely to occur relatively quickly following release, and dramatically decreases the longer one remains re-arrest or reconviction free in the community.

Lovell cites Lussier and colleagues (2011) to argue that repeat sexual offend-

ing is the likely driver of high prevalence rates of sexual offending overall. Lussier

and colleagues' study (2011, p. 435) involved 373 sexual offenders who had been

sentenced to at least two years. But this study was retrospective in design, and

focused on the sex crimes that *led* to the current conviction. Indeed, only a third of

the study sample had a prior sexual conviction on their record before the current

conviction, and thus only a third were *recidivating* when they committed the offense

for which they were incarcerated. Two thirds of the sample did not have a prior sex

crime conviction, and thus were reporting on their sexual offending that occurred

when they were still unknown to the justice system (i.e., in the 'general population').

Not only were the number of retrospective sex crime events much lower for

individuals who had a prior conviction than for 'new' offenders (Odds Ratio = .22;

Lussier et al., 2011, p. 438), but the offenders "without a prior conviction for a sex

crime (HR = 2.08) survived detection and sanction significantly longer" (p. 439). In

other words, the individuals who most closely match those listed on sex offender

registries – those with a prior sex crime conviction – not only were found to have

fewer instances of repeat undetected sexual offending (on average) than those

without prior convictions, but once they *did* commit a new sex crime, they were

caught much sooner than the 'unknown' offenders in the sample. This point was

reinforced later in the study: "Finally, offenders who managed to delay detection

longer were less likely to be known by the authorities. They spent less time in prison and are less likely to have a prior record for a sex crime." (Lussier et al., 2011, p. 442.) Put another way, "our study showed that sexual recidivists, or sex offenders with a prior charge for a sex crime, were the least productive (event-to-victim ratio) and were detected sooner than non-recidivists." (p. 442.)

This, again, suggests that the 'dark figure' of sex crimes would be smaller for individuals with a past sexual conviction (and thus likely listed on the registry) than for individuals who do not yet have a sexual conviction on their record. It also does not support any conclusion that the registry reduces sexual recidivism, since reported recidivism rates did not change when registries were introduced. Nor does it support any conclusion that individuals listed on the registry are responsible for a dispro-portionate number of undetected sex crimes, either overall or on a per-individual basis. In fact, it supports just the opposite conclusion.

> **Opinion:** *Self-reported sexual reoffending rates are much higher than sexual recidivism rates. Seminal studies of male commuter college students and military recruits found that between 62% and 71% of men who self-reported ever sexually assaulting, attempting to sexually assault, or using coercion to sexually assault, did so more than once.*

Reoffense rates of 'male commuter college students and military recruits' cannot be used predict reoffense rates for individuals with prior sex crime convic-tions. If anything, given their status as students and/or military recruits in their late teens or early twenties, it is more likely that most individuals in these two groups *do*

not have any prior convictions for sex crimes, and thus would be unknown 'new' offenders as far as the justice system is concerned. This again suggests that *unde-tected* sexual offenders with no prior criminal history (especially young males) are the more likely group to be engaging in repeat offending than people who have been convicted of past sexual offenses. This conclusion is seemingly supported by later findings cited in the Lovell study regarding SAK research, which I discuss in more detail below.

> **Opinion:** *[W]hat we know about sexual assault offenders is primarily based on those convicted of a sexual assault offense (the ~5%). Unfortunately, studies based solely on convicted offenders vastly under-represent sexual assault offending.*

This opinion highlights the faulty premise that much of the Lovell report is based on. That is, after lamenting the (factual) low reporting rate and high case attrition of sexual assault cases generally, Lovell then suggests that what *is* known about sexual assault offenders is based on those convicted of a sexual assault. Yet this is *precisely* the population at issue in *Does III* – individuals who have *already* been convicted of a sex crime. Lovell is right in the sense that we know much more about people with past convictions than about uncaught offenders.

In essence, Lovell is attempting to generalize research about the risk posed by 'uncaught' apples to the population of 'convicted' oranges. To state the point in a slightly different way, regardless of whether or not the registry works to reduce reoffending, the point of the registry is to protect the public from people *convicted*

of sex offenses. The registry cannot protect anyone from *unknown* sexual offenders who have not yet been detected by the police (and who were and are responsible for about 95% of reported sex offenses both before and after registries existed, see Sandler et al. 2008).

> **Opinion:** *Meta-analyses (analyses that examine a specific topic based on a large number of independently published studies to determine over-all trends), some of which were conducted by expert witness Dr. Hanson, found from the 1940s through the end of the 1990s and early 2000s that over an observation period of approximately five-years, the sexual recidivism rate was, on average, 13%.*

There is no one sexual recidivism rate for all people convicted of sex offenses, and recidivism numbers based on relatively short follow-up periods simply do not predict reoffense rates during later "time windows." Recidivism estimates vary *tremendously* based on the population being discussed. Lovell admits as much, noting that across the 808 empirical studies contained in one meta-analysis, the rates of sexual recidivism rates varied from 0% to 68%, depending on what population was being studied. Lovell suggests that the Alper & Durose (2019) study finding an 8% sexual recidivism rate over nine years is most generalizable, but even that study looked only at prisoners, and did not include people convicted of sex offenses who were given non-prison sentences. Moreover, as noted above, the "time window" matters: recidivism rates in the first year are higher than those in years 2-5, which are higher than those in years 5-9, which are higher than years 10-15, etc. (see Hanson et al, 2018; Thornton et al, 2021). This is reinforced by the fact that risk

levels fall based on years offense-free in the community (see Hanson et al., 2018.)

Indeed, Thornton and colleagues (2021) found that across 20 different samples

involving adult males with an officially recorded history of sexual crime and a valid

Static-99R score, each year offense-free in the community corresponded to a ~12%

decrease in the chances an individual would be arrested or convicted of a sexual

offense within the next year.

In addition, there is reason to believe that sexual recidivism rates today are

lower than they were from the mid-1900s through early 2000s, due to the dramatic

changes in society over the last 80+ years. For instance, some actions that were

historically considered to be sex crimes are no longer (e.g., consensual sodomy; see

Weinmeyer, 2014); technological changes have reduced barriers to distributing or

accessing child pornography; and other crimes may be more or less likely to be

prosecuted (e.g., statutory rape; see Olszewski, 2005; Cocca, 2002, 2004).

> **Opinion:** *Data from SAKs provide a more accurate discernment of the extent and nature of repeat sexual assault offending than data using only official administrative records of sexual recidivism.*

> **Opinion:** *Using data from the SAK Initiative, my research team and I conducted a study examining the offending behaviors of a large number of undetected sexual assault offenders (those not previously arrested and/or convicted of the sexual offenses associated with the kits), which allowed us to overcome the limitations of relying solely on official general recidivism to examine the offending behaviors of sexual assault offenders.*

While sexual assault kits (SAKs) may provide another method to determine repeat sexual offending via matching DNA profiles (including for uncaught perpetrators), I do not agree that it provides a 'more accurate' account of repeat sexual assault offending generally, due to the major limitations of the use of SAKs.

First, SAKs are used only in certain kinds of cases. The likelihood of a SAK being collected at all is highly variable, not equally spread across all sexual assault cases or victims, and not generalizable to all sex crimes. Lovell herself reports that only 50% of *reported* sexual assaults in Cleveland have SAKs collected. (Lovell Report, section 9.c.ii, p. 8, see also Campbell et al., 2001.) SAKs are simply unavailable for many types of sex offenses, including non-contact offenses (e.g., exhibitionism, possession of sexually abusive materials). SAKs are less likely to be used for lower-level contact offenses (e.g., touching over a person's clothes) because it is more difficult to obtain DNA. Where SAKs are collected, they are typically done soon after the assault, prior to any arrest or conviction. Due to the time-sensitive DNA collection window, SAKs often cannot be done unless the offense was recent, and thus they only reflect the types of offenses that victims are more likely to immediately report, which is not a representative sample of sexual offending. Many (though not all) SAKs relate to an offense committed by an unknown assailant. But research consistently shows the vast majority of sexual offenses are committed by family members, intimate partners, and other persons otherwise

known to the victim (see Smith et al., 2017; Berliner, et al., 1995; Bureau of Justice Statistics, 1997, 2000). Indeed, offending against either an unrelated or a 'stranger' victim increases a person's risk score on risk assessment instruments like the Static-99R. Thus, SAKs are disproportionately used in cases involving strangers, an inherently high-risk group that commits a far smaller percentage of sexual offenses.

Second, Lovell's discussion of SAKs presents another example of uncaught apples being compared to convicted oranges. Specifically, SAK research by Lovell et al. (2020, p. 476), notes that "a suspected serial sexual offender is defined as having at least one arrest for rape in addition to the SAK-associated sexual assault(s)." Since this does not measure convictions, nor does it even specify whether the other offense occurred *before* or *after* the SAK-identified offense, these findings cannot inform on the reoffense *or* recidivism rate of individuals who have a prior sex crime conviction. Indeed, of the 408 'suspected serial offenders' identified through the SAK research, 123 (30.2%) had been arrested for a rape that was not associated with the SAK. It is unclear what proportion of these 123 individuals had a prior sex crime conviction at the time of the SAK-identified assault. Lovell et al. (2020, p. 481) does note that "The vast majority [of suspected sexual perpetrators] continued to offend after the SAK-associated sexual assault and, more often than

not, do not have an arrest for rape in their criminal histories." [2] Thus, it appears that the vast majority of SAK-identified offenders were not 'known' (i.e., convicted) sexual offenders at the time of the SAK-identified crime. In other words, they were not like the population at issue in this case: people convicted of sex crimes. At bottom, Lovell wants to ascribe the level of DNA-detected serial offending found in this unrepresentative (and likely higher-risk) sampling of mostly uncaught offenders to all people who have convictions for sexual offenses.

Finally, it is unsurprising that SAKs identify the DNA profiles of repeat offenders (even if their identity remains unknown) because we know that some people commit serial offenses and don't get caught. SAKs are thus very useful to law enforcement to identify uncaught offenders. But because serial predatory rape by strangers is rare, and most sexual offenses are committed by people known to the victim, SAK linkages tell us nothing about the rate of undetected sexual assault by Michigan's 50,000+ registrants, whose sentences run the gamut from probation to life, and whose scores on risk assessment instruments vary widely.

> **Opinion:** *Reliable estimates of repeat sexual offending cannot be based solely on official sexual recidivism data.*

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[2] While Lovell et al. (2020, p. 478) notes that 7.6% of offenders have arrests only prior to the SAK-identified crime, and 56.1% of offenders have arrests both prior and post, it is unclear how many of these 'prior' arrests were for sex crimes, and how many resulted in a conviction.

While it is true that reliable estimates of repeat sexual offending cannot be based solely on official sexual recidivism data, what is *also* true is that reliable estimates of repeat sexual assault offending cannot be generated using SAK data, due to the aforementioned limitations. For example, the data noted in the Lovell Report (section 9.b.i, p. 6) indicates that 35.7% of the sample of suspected sexual offenders in Wayne County had DNA matches across multiple SAKs ("kit-to-kit" matches). Yet this says nothing about whether these 'serial sexual offenders' had *any* repeat sexual offenses after their first *conviction* for a sex crime – which would be the only type of repeat sexual offense relevant to the population covered in the *Does III* case. This is also an issue for the data noted in Cuyahoga County (section 9.b.ii). Specifically, the 30% of sampled individuals who had a *subsequent* rape offense had the initial starting event as the first coded SAK, not the first *conviction*.

In this same section, Lovell provides two anecdotal cases of serial sexual offenders. While SAKs clearly were helpful in identifying the 'full extent' of these individuals' *past* criminal actions, these cases say absolutely nothing about the sexual recidivism *or* sexual reoffending of people who have *already* been caught and convicted of a sex crime. Highlighting two heinous cases from over 800 cases in the dataset suggests that these are outliers, and their purpose is not to make a scientific point but to provoke fear and loathing in the reader.

> **Opinion:** *While these repeat sexual offending estimates from Wayne County and Cuyahoga County are much higher than the general sexual*

> *recidivism estimates of 8%, they are likely still undercounting the rate of serial sexual assault offending for several reasons.*

Opinion: *These estimates—35.7% (Wayne County) to 39% (Cuyahoga County)—should be considered the lowest reasonable estimates of repeat sexual assault. The "actual" sexual assault reoffending estimates are likely higher. However, how much higher is unknown.*

Here Lovell again mixes apples and oranges in attempting to address recidivism estimates. The data referenced from Wayne County and Cuyahoga County involve determining serial sexual offending among the population of individuals who were matched to at least one prior SAK, i.e., people who commit the types of offenses that are likely to result in SAK collection in the first place. But this is not the same population as individuals who commit a sexual offense *after being convicted for an earlier sex crime*, which is the population at issue here. In short, Lovell focuses on a modest, unrepresentative sample of largely undetected serial offenders, and then proclaims that the risk figures should apply to everyone anywhere who has ever been convicted of a sexual offense.

Opinion: *Women are more frequently raped by someone known to them versus unknown to them.*

This is an uncontroversial fact. It also reinforces the point made above about the unrepresentative nature of SAK studies. The Cuyahoga County study suggests that SAKs disproportionately sample stranger-based sex crimes (39.8%), compared to the prevalence of stranger rapes in the general population among women victims (12.1%). Thus, it is not surprising that Lovell finds a high rate of repeat sexual

offending among the SAK data, as it disproportionately covers (uncaught and higher risk) stranger offenders, which undermines the generalizability of SAK findings.

> **Opinion:** *Rape produces an extremely high societal cost of rape—the second most expensive violent crime after homicide.*
>
> **Opinion:** *A cost-effectiveness study my team and I conducted on the SAK Initiative in Cuyahoga County estimated the total cost of rape to victims whose SAKs were included in the SAK Initiative to be $241,960 per rape (adjusted for 2019 dollars), 38 based on commonly cited estimates of the cost of rape to victims.*
>
> **Opinion:** *Thus, preventing subsequent sexual offending results in significant societal cost savings for victims.*

Lovell provides estimates related to the tangible and intangible 'costs' of rape (Wang & Wein, 2018), and uses these estimates to conclude that preventing subsequent sexual offending would provide significant cost savings for victims. On its face, the conclusion that preventing sexual offending would result in cost savings is not controversial. Clearly one could raise reasonable questions about exactly how the 'intangible costs' are calculated (particularly given that they are over 35 times larger than the tangible costs [$241,960 per rape, with $235,363 intangible vs. $6,597 tangible]). These cost estimates are also presented for rape, but that does not necessarily generalize to other sex crimes (e.g., unwanted touching). However, the underlying idea that preventing sex crimes is a good thing for society is not controversial.

What *is* controversial is the use of these cost estimates as justification for the sex offender registry. Nothing in the Lovell report provides any evidence that the *registry* reduces sexual offending (either 'caught' or 'uncaught').

<div align="center">****</div>

In sum, after reading the Lovell report, one can conclude that SAKs are an important tool for the criminal justice system to identify and catch serial sexual offenders. But nothing in the Lovell report provides evidence that can be generalized to the population at the center of this case: Michigan's 50,000+ registrants, all of whom have a prior conviction for a sex crime and have been punished for it. Indeed, if anything, the research cited by Lovell suggests that the dark figure of sexual offending is significantly larger for people who have *never* been caught and convicted of a sex crime, compared to those who *have* been caught and convicted of a sex crime.

In discussing the benefits of SAKs, the high number of kit-to-kit matches, and matches of DNA to crimes that occurred *before* an individual was ever convicted of a sex crime, the Lovell report supports the idea that to increase public safety, resources should be increased for programs that encourage the reporting, investigation, evidence collection (including SAKs), and prosecution of sex crimes. Indeed, the resources devoted to sex offender registration would be put to better use supporting the ideas the Lovell report promotes.

Statements Expressed by Goodman-Williams and My Responses

> **Opinion:** *Sexual assault is pervasive; national studies indicate that approximately one in five women and one in thirty-three men experience attempted or completed rape at some point in their lives.*

Similar to the Lovell report, data about the *victimization rate* does not help one to determine the *sexual recidivism* (*or* sexual reoffending) rate or risk posed by individuals who have already been convicted of a sex crime and released back into the community. Even if we knew with certainty that 20, 50, or even 100 percent of women experienced an attempted or completed rape (however defined) at some point in their lives, that would *still* not answer the question of who was responsible for *committing* such crimes (i.e., *people with past sex offenses* versus *people without such convictions*). Further, it also does not demonstrate that registries have any measurable effect on reducing sexual offending (detected or otherwise).

As with Lovell, the definition of 'rape' is not consistent among the studies cited, and what is included influences the results. For example, in the national study cited by Goodman-Williams regarding the one-in-five estimate for women (Smith et al., 2017, Table 3.1), includes a category for *completed alcohol/drug-facilitated penetration* (about 1 in 11 offenses), which itself ranges from assaults committed using 'date rape' drugs to sex between intimate partners who are *both* "black out drunk" after a night on the town where *neither* party could legally consent to sex.

(See the 2010 NISVS codebook, at https://doi.org/10.3886/ ICPSR34305.v1). In

other words, the 'one-in-five' estimate includes a wide range of offenses, some of

which may not match the public's idea of what is being measured by the term 'rape'.

> **Opinion:** *Sexual assault victims have a history of being treated poorly by the criminal legal system (and society as a whole) when they report their assaults to police, with some victims describing experiences so painful that they consider them to be a "second rape."*
>
> **Opinion:** *Largely due to fears about how they will be treated if they report the assault and skepticism about the likelihood of positive outcomes, the vast majority of victims (between 75% – 85%) do not report their sexual assaults to police.*
>
> **Opinion:** *These concerns are well-founded; research has consistently found that for decades, sexual assault cases were routinely closed with minimal investigative work and most cases are not referred by police to prosecutors.*
>
> **Opinion:** *Attrition throughout the criminal legal system means that few reported sexual assaults (approximately 6.5% - 14.0%) end in a conviction.*

While these claims support the idea that more resources should be devoted to

better support and education around victim reporting, better treatment of victims

who report such crimes, better investigation of sexual assault cases, and better

prosecution, they have no relevance to the present case. That is, none of these claims

speak to the risk posed by individuals with prior sex crime convictions compared to

people in the general population, nor to the efficacy of the registry. The same as

above, whether the number of unreported, under-investigated, or non-prosecuted

cases is high or low, Defendants' expert cites no data studies to show that the registry

itself has changed (or can change) these numbers.

> **Opinion:** *Many of the cases that do end in a conviction reach that verdict through a plea bargain, meaning that the offender's criminal history record may not reflect the sex crime for which they were originally reported.*

Goodman-Williams suggests that recidivism research does not account for the

fact that some 'sex crime' charges might be pled as non-sex crime convictions. That

criticism simply does not apply to research that measures sexual offending using

arrest data, which has been done in many recidivism studies. (See, e.g., Sandler and

colleagues [2008].)[3] The use of *arrest* data to measure recidivism removes the

potential of missing individuals who plead to a non-sex-based crime.

> **Opinion:** *Collectively, these patterns of underreporting and case attrition limit the conclusions that can be drawn about serial sexual offending from criminal history records alone.*

> **Opinion:** *More specifically, estimates of serial sexual perpetration that rely solely on criminal history records are likely to substantially underestimate serial sexual offending.*

It is uncontroversial that sexual crime (like virtually all categories of crime)

is underreported and that there is case attrition in the criminal justice system. But

criminal history records underestimate repeat sexual offending both for individuals

who have been convicted of sex offenses in the past (i.e., the caught apples who are

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[3] Indeed, as noted by Sample and Bray (2006, p. 92), rearrest is ""one of the most common measures found in recidivism research."

on registries), *and* for people who have not been convicted of sex offenses (i.e., the uncaught oranges who are not on registries). Goodman-Williams repeatedly uses the term 'serial offenders,' by which she apparently means people who commit multiple offenses. But for purposes of registry analysis the question is not the extent to which people who commit sexual offenses commit multiple offenses. Rather the central questions are: how likely are people to commit new sexual offenses after they are arrested, convicted, and punished, and how does that compare to the offense rates of people who don't have convictions and whom we don't place on registries?

Underreporting/case attrition only matters if the underreporting/case attrition is higher for registrants than for non-registrants. Goodman-Williams presents no evidence to suggest that it is. If anything, her report suggests the opposite. Indeed, the studies cited by Goodman-Williams can be summarized as showing: 1) the vast majority of serial sexual assault offenses are committed by individuals with no prior sex crime conviction (i.e., offenders who are new to the criminal justice system and who are not on a sex offender registry), and 2) a small number of very high risk sex offenders (e.g., 'sexual psychopaths') may commit many sex crimes during their lifetime, but not necessarily after their initial sex crime conviction. Moreover, the studies cited by Goodman-Williams as evidence of a high rate of undetected 'serial

sexual offending'—which generally are based on samples of high-risk popula-

tions—have substantial problems in terms of generalizability to individuals with

convictions for prior sex crimes.

> **Opinion:** *Incorporating DNA linkages as indicators of serial sexual perpetration can mitigate the limitations of criminal history records and provide a fuller picture of serial sexual offending.*
>
> **Opinion:** *Research that incorporates DNA linkages as an indicator of serial sexual perpetration has found substantially higher rates of serial sexual perpetration than research that relies solely on criminal history records. More specifically, research that includes DNA linkages has found serial offending rates of 35% -39%.*
>
> **Opinion:** *Based on this research, I conclude that rates of serial sexual offending based solely on criminal history records substantially underestimate the frequency of serial sexual offending. Research that includes DNA evidence indicates that serial sexual offending is much more common than research based solely on criminal history records would suggest.*

As noted above, the population covered by DNA linkages is not representative

of the population on registries, and Goodman-Williams provides no evidence that

the rates of the DNA-linkage-population can be generalized to the diverse population

on registries who have committed a wide range of offense and have, in many cases,

not been arrested for or convicted of a new offense in decades. Even more important-

ly, also as noted above, while criminal history data understates the actual amount of

sexual offending, what matters here is the comparative rates of offending by previ-

ously convicted and unknown offenders. Much of the research on DNA linkages

suggests that most undetected serial sexual offending involves individuals who have

not yet been caught and convicted. In other words, the research seems to suggest that it is *not* individuals with a prior sexual conviction who are responsible for most serial sex crimes, but rather people not yet known to the justice system. As a result, this research does not provide any support for registries, *nor* does it inform on the risk posed by those with prior sex crime convictions.

The 'thought experiment' outlined by Goodman-Williams starting at paragraph 26 (p. 12) attempts to show that many individuals who offend repeatedly will not be identified as repeat offenders. Not only is that experiment based on a whole series of assumptions, but it again equates multiple offending before a criminal justice sanction with repeat offending after a sanction without any evidence to suggest those are the same. Thus, Goodman-Williams' example of hypothetical probabilities is irrelevant to the present case.

> **Opinion:** *There is no data, to my knowledge, on conditional arrest rates (i.e., the probability of a second report for sexual assault resulting in an arrest if the individual has been arrested for a previous sexual assault), so for the sake of argument let's suppose that it is 100%--that is to say, if someone commits two assaults, and they were arrested for the first one, we will suppose that they will always be arrested for the second.*

While not specific to sex crimes, Stolzenberg et al (2020) found that a prior criminal record increases the likelihood of police deciding to arrest an accused suspect. In other words, given two crime suspects who look identical and whose accused crimes are identical, police are more likely to arrest the suspect with a prior

record. If these results apply to sex crimes, then it suggests that the 'dark figure' of sex crime is likely larger for suspects *without* a prior conviction than for suspects *with* a prior conviction (which is the population at issue in the present case).

> **Opinion:** *Furthermore, the practical purpose of the steps that have been discussed thus far—from the police officer's decision of whether to thoroughly investigate, to their decision of whether or not to arrest, to the prosecutor's decision of whether or not to charge, to a judge or jury's decision regarding whether or not to convict—is to ensure that alleged perpetrators are only brought to court and potentially held accountable for sexual assaults in which there is exceedingly strong evidence against them.*

In this acknowledgement, Goodman-Williams undercuts much of the earlier argument about the 'problems' of high case attrition. That is, Goodman-Williams admits that the screening out of cases via subsequent steps in the justice system (investigation, arrest, charging, going to trial) is meant to ensure that alleged perpetrators are brought to court when there is exceedingly strong evidence against them. This is precisely the purpose of having a justice system based on 'innocent until proven guilty,' in which the prosecution must convince a jury that a crime has been committed beyond a reasonable doubt, and it is required for every kind of criminal offense.

While certainly more resources could be devoted to encouraging reporting, investigating, and pursuing charges, one must *also* ensure that those accused of crimes are provided with appropriate legal protections. The fact that the justice system acts as a 'funnel' to remove some cases during the process is inevitable.

Moreover, while this 'funneling' means that some alleged crimes are not contained in official criminal history records, the fact that there is case attrition before charges are brought is not a problem that can be fixed by a registry, and therefore is not relevant to the issues before the Court.

> **Opinion:** *What's more, when considering the self-report sexual perpetration literature as a whole, it appears that when people who perpetrate sexual assault (whether incarcerated, in sex offender treatment, or in the community at large) are given the opportunity to disclose the full range of their sexual perpetration behaviors with no legal repercussions, most disclose a great many more sexual assault perpetrations than they have ever had to criminally take responsibility for.*

To reach this conclusion, Goodman-Williams looks at two types of self-reporting studies. First, Goodman-Williams discusses two studies which she herself concedes involved samples of "particularly high risk offenders, and their rates of undetected offending should not be assumed to be generalizable to sex offenders more broadly." The first study, a 41-year-old publication by Groth and colleagues (1982), looked at convicted and incarcerated rapists and child molesters, some of the most dangerous sex offenders. The second study, a 32-year-old report by Weinrott and Saylor (1991), looked at "institutionalized 'sex psychopaths'" (p. 297) "committed for an indeterminate period in a civil proceeding" (p. 289). Neither study distinguished offending *before* and *after* an initial sex crime conviction. Decades-old studies that report on *lifetime* offending (detected and undetected) – rather than undetected offending occurring *after* an initial sex crime conviction – and that are

conducted on relatively small samples of high-risk offenders, cannot be generalized to the present-day population of diverse registrants who have been previously convicted for a wide range of sex crimes. As noted by Weinrott and Saylor (1991, p. 298), "All self-report studies to date [prior to 1991] have dealt exclusively with sex offenders involved in treatment. In addition, they have focused only on offenses prior to conviction."

Goodman-Williams describes the second set of (somewhat more recent) studies as being carried out on "general population samples." But the two studies are not actually general population samples. Rather one of the studies cited involves a survey of students at an urban commuter university, and another involves enlisted Navy personnel. Importantly, neither study sample likely involved people who *already* were convicted of sex offenses (nor was this measured). What these studies found is that 6.4% of the male college students and 13% of the Navy personnel had carried out behaviors meeting legal definitions of rape or attempted rape. While Goodman-Williams focuses on the percentages of people who reported multiple offenses, what is more relevant here is what these studies suggest about the offense rates of people *without* past sex offense convictions. That is, recidivism rates of between 5-15% are used to justify lifetime registration of people with sex offenses, yet the offense rates of the young men in these surveys was in a similar range (6-13%). Does it then follow that registries are needed to protect the public from male

college students and/or military recruits? Indeed, youth is a significant factor in predicting sexual offending or recidivism (see Doren, 2006; Zgoba et al., 2012; Wollert et al., 2010). What these surveys suggest is that young men may present more of a risk for sexual offending than many people who are on registries.

Opinion: *Whereas findings from recidivism studies typically estimate that 8% – 15% of sexual offenders recidivate, findings that incorporate CODIS data indicate that approximately 40% of sexual offenders sexually assault multiple victims. Based on the limited vantage point held by recidivism studies that rely solely on criminal history records, they are likely to substantially overestimate desistance among sexual offenders.*

Opinion: *Recidivism studies based solely on criminal history data are therefore not measuring sexual offenders' behaviors so much as they are measuring the behavior of the criminal legal system; they are not effectively measuring multiple instances of sexual offending so much as they are measuring multiple instances of being caught and held accountable by a system with a uniquely poor track record of doing so. Measures of serial sexual offending that include data less vulnerable to case attrition suggest that approximately 40% of sexual offenders are serial sexual offenders.*

Goodman-Williams fails to define the terms "sexual offender" or "serial sexual offender," yet concludes that 40% of sexual offenders are serial sexual offenders. That conclusion is unsupported and unsupportable.

First, the term "sexual offender", as used in the context of sex offender registration, includes a wide range of behaviors, including everything from sexual activity with a willing underage partner, non-contact offenses, and forcible rape. One simply cannot draw reliable conclusions about everyone who has committed a sex

offense based on research about those who committed the types of offenses that allow for DNA linkages or self-reporting by diagnosed psychopaths.

Second, as noted above, Goodman-Williams equates the research on unde-tected offending to undetected offending *after a conviction for a sex offense*. Multi-ple sexual offenses that occur at any point during an individual's lifetime is very different from a *reoffense* occurring after an initial conviction for a sex crime. As such, while Goodman-William's research may suggest that some people commit multiple offenses against multiple victims, it does not inform at all on the likelihood of either sexual recidivism, or of sexual reoffending, for people with a prior sex crime conviction. Suggesting that "approximately 40% of sexual offenders are serial sexual offenders" (i.e., have had more than one victim), is not the same as estimating 40% of individuals *with a prior sex crime conviction* go on to commit another sex crime. While it may be that some people "who sexually offend once often do so multiple times and against multiple different victims," this says nothing about the reoffending of people who commit a sexual offense and are caught, convicted, and punished for it.

Statements Expressed by Salter and My Responses

> **Opinion:** *Claiming that an instrument which is based on recidivism is actual-ly measuring reoffending misleads the reader into thinking that sexual reoffending relapse rates are low. In reality, because of the*

> *large number of undetected offenses, there is no reliable infor-*
> *mation on reoffending rates.*

It is true that official recidivism measures (i.e., rearrest, reconviction) are not the same as reoffending, and that there is no reliable information on reoffending rates (which is why most scholars use recidivism rates). Nevertheless, it is possible to show that policies such as the registry appear to have no impact on *either* the recidivism rate *or* the reoffending rate.

Salter herself cites research by Sandler and colleagues (2008) showing that the vast majority of sex crime arrests are of previously unknown offenders. Salter says this is a 'red herring' (Salter Report, p. 10), because "there is no compelling evidence to suggest the 14% of [offenses reported to police] is different between those with no previous convictions and those who do" [*sic*]. (Salter Report, p. 10.) Yet, if the ratio of reported to unreported offenses is the same for people with and without past convictions, that means that, whether the rate of unreported offenses is low or high, approximately 95% of those offenses are being committed by people who are not on registries.

Moreover, the findings of Sandler and colleagues (2008), which I coauthored, bear on the effectiveness of the registry for individuals both *without* prior convictions and *with* prior convictions. Specifically, our research concluded that New York's public sex offender registry did not change *either* the rate of arrests for *new* sex offenders (i.e., those without a prior sex crime conviction at the time of arrest),

or the rate of arrests for *recidivating* sex offenders (i.e., those with a prior sex crime conviction at the time of arrest). If the New York registry was effective, then one would expect to see *some* change in either (or both) of these sex crime rates. This would be true whether the proportion of sex crimes that go unreported or do not result in an arrest are the same *or* different between these two groups.

This is because any general deterrent effects of the registry would necessarily involve fewer overall sex crimes being committed by *new* offenders. One would expect that at least *some* of the sex crimes that did not occur due to general deterrence would have otherwise resulted in an arrest. Therefore, any general deterrence effects should still be measurable as some type of reduction in the 'new offender' arrest rates. Yet we did not find any such reduction, which led us to conclude that the registry did not deter sex crimes committed by new offenders (i.e., those not previously known to the justice system).

The same is true for measuring any reductions in the *recidivist* sex crime arrest rate, due to possible specific deterrent effects of New York's registry. That is, if the registry resulted in fewer sex crimes being committed *by people who had a prior sex crime conviction*, then some portion of these deterred recidivistic sex crimes would have otherwise led to an arrest, and therefore one should see some type of reduction in the 'recidivistic' sex crime arrests. We did not find any such reduction, which

suggests that the registry also did not deter sex crimes committed by people with a prior sex crime conviction, either.

Interestingly, if the registry were effective at facilitating the investigation and prosecution of sex crimes, either through increased reporting by victims or through aiding police in identifying suspects, one would expect the registry to *increase* sex crime arrests, either for 'new' or 'recidivist' offenders. In other words, if the registry itself led to a reduction in the 'justice gap' between crimes occurring and crimes resulting in an arrest (and thus reducing the size of the 'dark figure'), this should have been identifiable as *increases* in sex crime arrests for either (or both) offenders with prior convictions and/or those without. Yet we found neither outcome, which led us to conclude that New York's registry did not have any effect in terms of increased reporting *or* successful investigations.

> **Opinion:** *Claiming that reoffending rates are known and are low prevents the public and policymakers from grappling with "the justice gap," the gap between the number of offenses that are committed and the number that result in charges and/or convictions.*

Salter cites the 'justice gap' as the difference between crimes that occur and crimes that result in charges or convictions. Salter then suggests that the public and policymakers are hindered in addressing the "dark figure" of crime because of claims that reoffense rates are known and low. There are several problems with this argument.

First, research suggests that the public believes that sex crimes rates *and* the risk of sexual recidivism is exceedingly high (see CSOM, 2008; Katz-Schiavone, et al., 2008; Socia and Harris, 2016). This is supported by how the media covers sex crimes (Galeste, et al., 2016). Indeed, rather than being hindered, the public and policymakers have done an exceedingly thorough job at adopting a wide body of legislation pertaining to individuals convicted of sex crimes (e.g., registration, public notification, residence restrictions, GPS monitoring, travel restrictions, civil commitment), all of it premised on assumptions about high reoffense rates.

Second, while generating reliable estimates of *reoffending* rates is difficult, and while official recidivism rates necessarily underestimate actual reoffending, nothing in the Salter Report suggests that the registry is effective at reducing either recidivism or reoffending, nor does it provide any evidence that the registry is effective at increasing victim reporting or police investigation success. In short, the underlying basis for the current court case, the efficacy of the registry at reducing sex crimes, is not addressed by anything contained in the Salter Report.

> **Opinion:** *Recidivism rates cannot be used as a proxy for reoffending rates because of differential reporting. Victims report some types of offenders, e.g., extra-familial and stranger offenders, more than they do other types, e.g., familial and familiar. This leads to some types of offenders having higher recidivism rates than others, although they may reoffend the same amount or more.*

While it is true that certain types of victims (e.g., where the perpetrator is outside the family, or is a stranger) are more likely to report sex crimes committed

Socia Rebuttal, p. 40

against them, what this means is that recidivism estimates cannot be taken as one-size-fits-all. Rather than supporting the need for the registry, this claim instead supports the use of actuarial risk scores and criminal history records to make individualized assessments of risk, rather than treating all people with sex offenses the same.

> **Opinion:** *The second counter is that sanctions make a difference and offenders commit fewer offenses once they have been sanctioned. The issue of sanctions was addressed by a recent study by Kelly [sic] (in press). This study looked at 200 men who were ultimately civilly committed as Sexually Violent Predators. Polygraphs, done after their commitment, focused on detected and undetected offending in their past and correlated the number of undetected offenses with the dates of the sanctions…. I offer it, [citing Kelley et al. (2022)] not as an indicator of how many undetected offenses the average offender commits, but because this study is used to say that undetected offending goes down after sanctions and that sex offenders are not likely to have many undetected offenses after a sanction. In this study, at all arrests and sanctions, there were more undetected offenses than detected.*

The study Salter discusses, conducted by Kelley and colleagues (2022), was of 200 men who were civilly committed as Sexual Violent Predators (SVPs).[4] As Salter admits (p. 11), this is not a typical sample, as these individuals were *already* identified as exceedingly high-risk (as indicated by the successful civil commitment procedures), and thus this sample cannot possibly be generalized to all individuals

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[4] As someone extremely familiar with Kelly as a name, I must point out that the last name of the primary author is spelled Kelley, not Kelly, as cited by Salter. While the study might be considered 'in press', it is available online as an *OnlineFirst* article, and thus is cited here using the year it became available online (2022).

with a prior sex crime conviction (and thus listed on registries).[5] Salter focuses on the fact that there were more undetected offenses than detected offenses in the Kelley et al. (2022) sample, while simply dismissing two of the main findings of the study that undercut her entire argument. Specifically, the study showed that even in this *extremely* high-risk sample: 1) undetected offenses *decreased* after the initial sanction, and 2) most of the individuals in this sample *did not* have a very high number of undetected offenses.

First, Kelley et al. (2022) found that undetected offending by individuals in the study had *decreased* after the initial sanction for a sex crime (i.e., the proportion of detected offending increased). Kelley also found that the proportion of offenses that were detected was significantly higher after the initial sanction: among the entire sample of eligible individuals (N = 189), the proportion of offenses detected prior to the first arrest was about 23%, but rose to about 36% after the first period of post-conviction release.

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[5] As noted by Kelley et al. (2022, p. 6), "As demonstrated by their Static-99R scores (M = 6.52, SD = 1.92) at the time last taken into custody, this was a well above average risk sample." The laundry list of mental disorders coded in the sample further indicate that the sample is not generalizable to the broad population of registrants. Specifically, Kelley et al. (2022, p. 6) note the following mental disorders were coded in the sample: "Pedophilic Disorder (49.5%), Other Specified Paraphilic Disorder with hebephilic features (10.5%), Other Specified Paraphilic Disorder with coercive features (13.5%), Sexual Sadism Disorder (14.0%), Antisocial Personality Disorder (55.5%), any Substance Use Disorder (49.5%), any major mental illness including Bipolar Disorder Type I and psychotic spectrum disorders (8.5%), and Intellectual Disability (7.5%). An 'Other' category captured diagnoses not otherwise coded, such as depressive or anxiety disorders (42.5%)."

Second, Kelley (2022, p. 11) noted that "Most cases reported no undetected victims (34.0%) or between one and five undetected victims (40.7%)." While the generalizability of this high-risk sample is questionable, it supports the claim that the proportion of undetected sex crimes (the 'dark figure') is likely *smaller* for those who have been convicted of a first sex crime, compared to those who have not yet been convicted of a sex crime. Thus, the findings of Kelley et al. (2022) *directly contradict* Salter's earlier claim (p. 10) that "All in all, there is no compelling evidence to suggest the 14% of reported offenses is different between those with no previous convictions and those who do." Indeed, the Kelley et al. (2022) says that the proportion of undetected offending was lower (and, relatedly, the reporting rate was higher) for those with prior convictions compared to those without. Further, Kelley et al. (2022, pp. 2-3) cites one of Defendants' own experts in making a related point about the offending differences between those with and without prior convictions: "[I]ndividuals with prior convictions for sexual offenses tend to have fewer reported victims compared to individuals who have never been arrested, such as college students (Lovell et al., 2020)."

Shortly before citing Kelley et al. (2022), Salter points to multiple studies involving general community crime reports to suggest a high rate of undetected sex crimes in the United States (see pp. 7-8). Yet Kelley et al. (2022, p. 18) suggest that

undetected sex crime rates drawn from unofficial community crime reports *overstate*

the risk of undetected *offending* among those with prior sex crime convictions:

> While individuals with prior detected sexual offenses will be identified
> as greater risk on actuarial instruments, their risk for engaging in future
> undetected sexual offending does not appear to be as dramatically high
> as general community crime reports have indicated.

This quote directly undercuts Salter's arguments that the proportion of undetected

sex crimes *in the community* is evidence that undetected offending *among those with*

prior sex crime convictions is equally high.

Opinion: *Weinrott and Saylor studied 37 rapists committed to a Western State Hospital for an indefinite period as "sex psychopaths." They had between them 66 official charges. Under conditions of confidentiality the 37 men self-reported 433 rapes. Thus they had an average of 1.8 official charges but self-reported an average of 11.7 victims, or 6.5 times more victims than their number of charges and/or convictions indicated (Weinrott & Saylor, 1991).*

Opinion: *Scurich and John are correct when they say, "Under any configuration of assumptions, the dark figure is substantial."*

Opinion: *DeLisi's 2016 study of 119 federal sexual offenders used self-report backed by polygraph. The ratio of self-report to official victims was 3.32. The seven most prolific sex offenders included four with no official criminal history except for possession or receipt of child pornography. Child pornography offenders pose a risk not just of future of child pornography possession but often have a history of contact sexual offending and a future risk, particularly in the early years of release, of future contact sexual offending.*

Opinion: *Bourke and Hernandez found in their study of child pornography offenders that 26% of their sample of child pornography offenders were known to have at least one hands-on victims at sentencing, whereas by the end of treatment and polygraph testing 85% had acknowledged hands-on victims (Bourke & Hernandez, 2009).*

Socia Rebuttal, p. 44

Salter cites a number of studies involving undetected offending among sex offenders (e.g., Drury et al., 2020; Delisi et al., 2016; Bourke and Hernandez, 2009; Weinrott and Saylor, 1991), as apparent evidence that many people convicted of sex offenses have high rates of undetected sex offenses and pose a high risk for future sexual offending. Many of these studies measured *lifetime* undetected offending, which has no direct correlation with the risk of *reoffense* after an initial sex crime conviction. Additionally, many of these specific studies have other important limitations that are not fully disclosed by Salter, as noted below.

Weinrott and Saylor's (1991) study of sexual psychopaths

The study by Weinrott and Saylor's (1991) involved a very small sample of the most dangerous offenders, who were all committed to a State Hospital as 'sex psychopaths,' and who thus cannot be generalizable to Michigan's registrants. Moreover, the study included sex offenses committed prior to conviction, and thus does not cover sexual *recidivism*. How an institutionalized sample of 'sex psychopaths' is generalizable to individuals on the registry is unclear. Further, the authors noted that the "majority of offenses committed by both rapists and child molesters was of the nontouching variety: exhibitionism, voyeurism, and obscene phone calls." (Weinrott and Saylor, 1991, p. 299, endnote 1). To include everything from obscene phone calls to child sexual abuse in the definition of 'sexual offending' used to measure undetected offending has the effect of inflating the number of 'uncaught

crimes'. When combined with the sample being considered, the results of this study cannot be generalized beyond the walls of the specific State Hospital where the sample was obtained.

Scurich and John's (2019) estimates of 'actual sexual recidivism'

Scurich and John (2019) attempt to provide estimates of the "dark figure" of sexual *recidivism* (although this should rightly be referred to as sexual *reoffending*). To do this, the researchers combined victim self-report survey data, offender self-report data regarding the number of victims, and assumptions about the likelihood of conviction for a reported sex crime. Yet in combining these estimates/ assumptions, they provide a *wide range* of the estimated sexual "recidivism" rate that depends *heavily* on the underlying numbers being fed into their models. These numbers also assume that each sexual offense has a consistent probability of being reported *and* a consistent probability of resulting in a conviction or a plea. Yet these assumptions do not reflect reality. Thus, these estimates are not targeted towards estimates of *registrants'* sex crimes, but rather the sex crimes of *all* perpetrators, regardless of prior convictions or registration status (see Lave et al., 2021).

Finally, the researchers vary the *distribution* of offender's propensity to commit sexual offenses, with mean rates ranging from 1/16 to 16 sexual offenses per year. While this variation is indeed more advanced than assuming a consistent offense rate across all offenders, the authors note that the assumptions about

offending used in their model (i.e., the distribution of offending) "could be replaced with any distribution thought to describe sexual offending propensity for a particular population of offenders over a given period of time." (Scurich and John, 2019, p. 166.) Thus, the estimates of sex offending/recidivism provided by Scurich and John's (2019) model depends entirely on the assumptions/estimates being fed into the model, and changes in those estimates could dramatically alter the findings. Further, the actual range of estimates given by Scurich and John's (2019, p. 168) are not helpful for any practical purpose: their estimated 5-year recidivism rate ranges between *6% and 94%*. How this study, which is more an exercise in statistical modeling than anything else, realistically supports claims of high levels of undetected sexual reoffending, is unclear. Indeed, as Salter herself acknowledges, the Scurich and John study has been widely criticized for the assumptions and methodology employed. *See, e.g.* (Abbott, 2020; Lave et al., 2021).

Bourke and Hernandez's (2009) study on child pornography offenders

The Bourke and Hernandez (2009) study cited by Salter has similarly received substantial criticism. Indeed, the second author of that study (Hernandez) has noted that "the argument that the majority of [child pornography] offenders are indeed contact sexual offenders and, therefore, dangerous predators ... simply is not supported by the scientific evidence." (Hernandez, 2009, p. 4). The problems with this

study, and the misuse of it to suggest the potential dangerousness of child porno-

graphy offenders, have been pointed out by many scholars (*see* Hamilton, 2011; Seto

et al., 2011; Wollert and Skelton, 2017; U.S. v. Apodaca, 2011).

DeLisi and colleagues' (2016) estimates of the dark figure of sexual offending.

Finally, the use of Delisi et al. (2016) as evidence of a substantial dark figure

for sexual crime is highly problematic. Specifically, DeLisi and colleagues (2016)

attempted to estimate the dark figure of sex crimes using a sample of 119 individuals

with a federal sex offense conviction in the central United States between 2010 and

2015. It is unclear how the sample of 119 individuals was selected, and thus it is

unclear what broader population it is meant to generalize to. Still, and perhaps most

problematic, DeLisi and colleagues (2016) combined the sample's self-reported sex

crime data and known criminal history data with estimates taken from an *unrelated*

Canadian study (Mathesius & Lussier, 2014) regarding the number of 'hidden' sex

crimes, in order to estimate the 'dark figure of sex crimes.'[6]

Of the 119 individuals, DeLisi and colleagues (2016) identified 34 individuals

who had no official record of prior sexual abuse but did have a self-reported history

of sexual abuse. DeLisi and colleagues (2016, p. 10) then estimated that these "34

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[6] The Canadian study by Mathesius and Lussier (2014, p. 136) involved 332 first time sexual offenders who were "incarcerated for a sex crime and were sentenced to a Federal penitentiary in the province of Quebec between April 1994 and June 2000. All offenders were serving at least a two-year sentence for their crime." Thus, it is not clear that estimates from this sample of incarcerated Canadians can be generalized to DeLisi and colleagues' (2016) study sample.

clients offended against 148 victims that potentially denoted a minimum number of 148 sex crime events, a median number of 1,480 sex crime events, a mean number of 32,101 sex crime events, and *a maximum number of 827,552 sex crime events.*" (Emphasis added.) Thus, the estimate of the 'dark figure of sex crimes' stemming from 34 individuals is presented as somewhere between 148 and 827,552 sex crimes. In short, the 'estimate' is no more reliable than a wild guess at best.

As an indication of the absurd implausibility of this estimated range, let us take DeLisi and colleagues' (2016) maximum estimate of 827,552 sex crimes as being feasible (p. 10). If this is the case, then *each* of these 34 individuals would be responsible for 24,340 undetected sex crimes, on average. Let us further break this down into how long it may take to achieve this feat. Assuming an individual were to commit one such sex crime *every day, non-stop*, it would take each individual a total of 66.7 *years* to achieve 24,340 sex crimes. If these 827,552 sex crimes were not evenly distributed, then it would take some individuals even longer to achieve this feat (assuming a rate of 1 sex crime per day). The point of this exercise is to demonstrate that these huge ranges of 'hidden sex crimes' may seem frightening at face value, but when broken down and viewed with a bit of reality, they are no better than guesses. In this study the estimated range is so implausible that it cannot be taken as anything other statistical game-playing. Further, like almost all of the other studies

relied upon by Salter, none actually measures recidivism or *reoffending* after a sex crime conviction, and thus has little applicability to the present case.

Declarations of Non-Experts

The additional declarations from Dare, Bennetts, and Rennie present a variety of anecdotal cases, personal beliefs, and/or cherry-picked citations with little explanation that have largely been responded to earlier in this rebuttal when addressing the Defendants' experts' reports. To summarize a handful of the problems with these reports:

Dare's declaration largely relies on personal experiences as a prosecutor to make vast claims about "most instances of criminal sexual conduct" and the "likelihood of committing criminal sexual conduct" (p. 1). Anecdotal experiences and beliefs of a single prosecutor in Oakland County cannot be taken as generalizable, even to Oakland County sexual assault cases. Nor is it clear why one should give any credence to one prosecutor's "belief that alerting the public via the Sex Offender Registry about individuals that have committed past sex offenses, benefits the public because it encourages victims of criminal sexual conduct to come forward to report crimes." That is, without actual evidence to support this belief, it is nothing more than a personal opinion. Even the limited information Dare provides in table format about the criminal sexual conduct convictions of the Oakland SVU (p. 3) does not

have enough context to know why the comparison of 'Total Cases' to 'Total Convictions' is relevant to the present case.

Bennetts' declaration is noted as being based on 'personal knowledge,' and thus represents a similar set of anecdotal experiences and personal beliefs that have little generalizability. The statistics that are cited (largely from RAINN) relate to the prevalence of victimization, and say nothing about sexual reoffending and/or sexual recidivism. One example case Bennetts refers to a serial offender who "terrorized multiple cities in Wayne County for approximately five years before being caught." (p. 2). Apparently this individual was not on the registry and was not known to the justice system; his story is thus not relevant to the issues in the present case. Even the example cases "involving a sex offender who has reoffended while already on the SORA registry" (pp. 2-4) are cherry-picked anecdotes and offer no ability to generalize beyond those specific offenders. All they suggest is that listing the person on the registry did not prevent these offenses from occurring. Bennetts' statements *about* victims' personal perceptions of the registry's utility are similarly ungrounded. (p. 4). That is, without actual study data, simply saying things like "Many victims state" (p. 4) provides no ability to contextualize what 'many' means, whether these statements are generalizable, or even if they are based on actual evidence vs. mere perceptions.

Rennie's declaration relies on four total citations: Alper and Durose (2019), Przybylski (2015), The Detroit Sexual Assault Kit (SAK) Action Research Project (see Campbell et al., 2015), and the RAINN website. The limitations of SAKs like those used in the Detroit study have been noted earlier. The Alper and Durose (2019) research has also been discussed earlier, but largely shows relatively low rates of sexual recidivism (7.7% over 9 years). Further, the Przybylski (2015) report covers a range of individual studies loosely referred to by Rennie, most of which have already been discussed earlier in this Rebuttal, and most of which show sexual recidivism estimates (as measured by rearrest) in line with prior estimates already discussed (e.g., Sample and Bray, 2003). In responding to one particular 'secondary citation' referenced in the Przybylski (2015) report, a meta-analysis by Harris and Hanson (2004) found somewhat higher rates of sexual recidivism across long follow-up periods than other studies. Yet to cite the authors directly about what their study found: "Results indicated that most sexual offenders do not re-offend sexually, that first-time sexual offenders are significantly less likely to sexually re-offend than those with previous sexual convictions, and that offenders over the age of 50 are less likely to re-offend than younger offenders." (Hanson and Harris, 2004, p. ii). Further, "Given that the level of sexual recidivism is lower than commonly believed, discussions of the risk posed by sexual offenders should clearly differentiate between the high public concern about these offences and the relatively low

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probability of sexual re-offence." (Hanson and Harris, 2004, p. 12). Thus, the selective use of recidivism estimates referred to by Rennie largely ignores the context given by the authors themselves. Finally, references to the RAINN data, as noted earlier, involves victimization estimates and case processing outcomes, and has little relevance to issues in the present case.

Conclusion

Overall, when it comes to estimating the dark figure of sexual offending that is relevant for the current case – that of sexual reoffending following a prior sex crime conviction – the various estimates presented by Lovell, Goodman-Williams, and Salter are simply not reliable for this purpose. Further, in some cases (e.g., Salter's use of Kelley et al. [2022]), studies are seemingly mis-cited and/or some findings are simply ignored when they contradict the claims being made. The declarations of three attorneys (Date, Bennetts, and Rennie) are largely anecdotal experiences, personal beliefs, and/or cherry-picked statistics that have little relevance to the issues of the present case. Finally, the various declarations and reports by Defendant's experts do not dispute any of the conclusions of my initial report.

<u>Oath and Signature</u>

I declare under penalty of perjury under the laws of the United States of America

that the foregoing is true and correct to the best of my knowledge, information, and

belief.

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